

The gist of his argument lies in his assertion, that "if a true pound, as determined at London, were carried to the North Pole, it would weigh more than a pound."

Now, since the determination of a pound is actually effected by making it a copy of a standard so that they shall counterpoise each other *in vacuo*, it is strictly independent of locality.

The standards actually employed for this purpose, for example, the authorised copies of the principal standard pound which have been sent to various countries, have been made as nearly identical in mass as skilled workmen could make them, and have been sent at random to different latitudes.

Similar remarks apply to the weights in a chemist's box. Surely it is not seriously proposed that the chemist should file them down to accommodate them to the increase of gravity, when he takes them from London to Edinburgh.

Where should we find ourselves, if the makers of chemists' weights endeavoured to make them of different masses, according to the places where they were to be used?

Fortunately, this has never been attempted; and since all parts of the world are in possession of practically identical standards of mass, under the name of standard pound or standard kilogramme, and tolerably accurate copies of these and their fractional parts are in everybody's hands, why not acknowledge them as standards of mass, which they are in point of fact, although, according to the theory which I am combating, they ought not to be?

Those who hold that theory must choose between two evils:—they must either make the pound a unit of force, in which case they must file or load their weights as they go from place to place (this seems to be the alternative which W. M. W. chooses); or they must accept the gravitating forces of equal pieces of metal as nominal units of force at the different localities where these pieces of metal may happen to be, although these forces are really not equal. This latter alternative, which gives a variable unit of force, has been commonly adopted till recently, and a variable unit of mass has been conjured up to suit it.

If our spring-balances were as accurate as our standard weights, W. M. W.'s idea would be practicable. The equality of two forces at different places could then be very directly tested; but, in fact, the most accurate means we possess of making such a comparison consists in a double process, a weighing with the ordinary balance, combined with a difficult and less accurate pendulum-comparison. Inasmuch then as ease and accuracy of comparison is the first essential of a scientific standard, I submit that the world is right in employing standard pounds of equal mass and not standard pounds of equal gravitating force.

Much confusion arises from using the word *weight* in a connection which leaves it doubtful whether *mass* or *gravitating force* is meant. I trust you will keep your columns open for the further discussion of this question, as it much needs ventilation.

Belfast, January 11

J. D. EVERETT

The Tails of Comets, the Solar Corona, and the Aurora considered as Electric Phenomena

IN reference to a letter from Mr. Bedford, Ph.D., published in your last number, allow me to state—

1. That I had never seen or heard of Dr. Bedford's theories.
2. That, judging from the extract given in his letter, Dr. Bedford has not published anything analogous to the electrical hypothesis which I have put forward. In his letter he has misquoted and made omissions to the extent of one-half the heading, in order, I suppose, to avoid the very mention of the word electricity, which, on the other hand, is the very substance of my postulate.

Owens College, Jan. 17

OSBORNE REYNOLDS

Apparent Size of the Moon

FROM almost any place in the balcony of St. James's Hall, at an evening performance, one may find in the coronets of gas-jets, forming one or more of the arches across the roof, corroboration of what I wrote in NATURE of May 12, 1870 (vol. ii. p. 27). The nearest or first coronet, and the two next, observe a gradation of increase in apparent magnitude, as they should do. But the rest, which (unlike those three) descend towards the horizon, and should, nevertheless, observe a gradation of increase, are apparently all of a size. I call attention to this "unconsidered trifle" as having the full force of a very elaborate experiment.

C. M. INGLEBY

Ilford, Jan. 7.

Atmospheric Currents

THE following is part of a letter signed "G." in NATURE of 6th October, 1870:—

"It is very important to obtain correct and copious data regarding the atmospheric currents between say 5,000 feet and five miles above the level of the sea, and especially at various points on and near the equator, and at about 30° to 32° North and South latitudes. Within these limits the rain-bearing currents of the atmosphere move. If self-registering meteorological instruments were placed permanently upon several of the leading mountain ranges of the world, and their records copied at stated intervals, we should obtain valuable data for determining the direction, velocity, and magnitude of the controlling atmospheric currents of the globe."

This suggestion is most valuable, but it would involve a source of error that would be difficult to allow for. Most mountain ranges occupy so extensive an area that they have their own local climates, and indications on such ranges would consequently not give accurate information about the currents where they are not modified by such influences. The most valuable information will be obtained from the most isolated mountains. The most isolated mountains of sufficient height, within sufficiently easy reach of us, are Etna and Teneriffe. Etna is in the variables, and Teneriffe in the trade-winds.

I do not, however, agree with the following suggestion, that more valuable observations still might be obtained by means of captive balloons. Balloons cannot be used in stormy weather, and they are also subject to the great disadvantage, that they tell nothing about barometric fluctuations: because the height of a balloon above the earth can be known only from the barometer, and consequently there is no way of ascertaining the indications of the barometer at a known height. It is not at all certain that the barometric curves at the summit and at the base of Etna or Teneriffe would present any close correspondence. Sets of barometrical and other meteorological observations taken for sufficiently long periods at the summit and at the base of such mountains would be probably the most valuable of all data for meteorological science in its present state.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, Jan. 14

THE AMERICAN ECLIPSE EXPEDITION

THAT portion of the American Eclipse Expedition which was stationed at Xeres was favoured by weather which enabled the observers to examine the Corona during the whole period of totality, and with some results of interest; an informal account of a part of which the kindness of Professor Winlock enables me to lay before your readers.

The party was in charge of Professor J. Winlock, and stationed in an olive grove about a mile from the city, the property of Mr. Davies, of Xeres, to whom all the members of the expedition are indebted for continual aid and kindness. The observations for the determination of longitude, latitude, and times of contact were conducted by Assistant G. W. Dean, of the Coast Survey, aided by Captain Ernst, of the U.S. Engineers, and by Mr. Gannett, of the Harvard College Observatory.

Their labours were facilitated by the courtesy of the Director of the neighbouring Observatory of San Fernando, with which telegraph connection was established, and the results attained will be found in full in their official report.

Besides the instruments of precision, the party was provided with four equatorial telescopes of from six to eight inches aperture, driven by clockwork, and many smaller ones. One of the largest telescopes carried an objective specially corrected for actinic rays, with which, and a companion instrument, photographs were taken during the totality and previous stages by Mr. Willard and his assistant.

In addition, a photographic telescope of upwards of thirty feet focus, placed horizontally, and receiving the solar rays from a heliostat, was used, in charge of Mr. Gannett.

The accessories of the instruments were such as the previous experience of the observers (nearly all of whom had witnessed the eclipse of August 1869) suggested, and were too numerous for description here, though a means

of rapidly recording the position of the lines noted in the Spectroscope, devised by Professor Winlock, proved of too essential service to be left unnoticed. Each spectroscopic was provided before leaving America with this attachment, which enabled the observer to record the position of the lines as fast as he could point on them, and with a precision that compared favourably with micrometrical measurement.

I pass to the work done by each observer.

The Spectroscope used by Prof. Young was the one recently described in your pages; and through this he was enabled to watch the occultation of the protuberances of the photosphere, and announce the approach of the moon, some fifteen seconds before "first contact." At this time the sky about the sun was nearly clear, and numerous successive photographs were taken, with the accompanying chronographic record of the instant of exposure. Heavy clouds then obscured the sun, at intervals, till totality was very near, when a happy fortune gave us a few minutes of almost clear sky while it lasted.

It is important to explain that, though the sky was so little obscured that the Corona was visible during the whole of totality, there was during most of this time a very slight haze. I recall a short view of sky, distinctly blue in spite of the darkness; and the structure of the protuberances was continuously visible through the telescope; nevertheless, the presence of this partial haze should be remembered, as having a possible bearing on some of the conclusions to be drawn from what follows.

I give as nearly as I briefly can the principal results of each observer, as regards the corona only.

Professor J. Winlock, using a spectroscope of two prisms on a five-and-a-half-inch achromatic (directed by Mr. A. Clark as the finder) found a faint continuous spectrum, without dark lines.

Of the bright lines, the most conspicuous was 1474 Kirchhoff, which was followed all around the sun to at least 20' from the disc. It may be here remarked that all the spectroscopes showed this as much the most conspicuous coronal line. A number of other lines were also noted, and their position recorded by the apparatus just referred to.

Professor Young's observations of this line were similar; he estimates its least extension at one half the solar diameter. With the slit of his spectroscope placed tangentially at the moment of obscuration, and for one or two seconds later, the field of the instrument was *filled with bright lines*. As far as could be judged, during this brief interval every non-atmospheric line of the visible spectrum showed bright; an interesting observation, confirmed by Mr. Pye, a young gentleman whose voluntary aid proved of much service. The observation of Mr. Pye was made with a spectroscope of one prism, before which was placed a small telescope arranged at Prof. Young's suggestion, not to give, as usual, a definite image on the slit, but to supply light from all portions of the corona and neighbouring sky with more intensity than would be furnished by directing the instrument toward the sun without it. From the concurrence of these quite independent observations, we seem to be justified in assuming the probable existence of an envelope surrounding the Photosphere, and beneath the Chromosphere usually so called, whose thickness must be limited to two or three seconds of arc, and which gives a discontinuous spectrum consisting of all, or nearly all, the ordinary lines, showing them, that is to say, *bright* on a dark field.

Mr. Abney, of Wadham College, Oxford, assisted with a Spectroscope prepared by Prof. Young for collateral observations to his own, and obtained results which have already been made public.

The Polariscopes were used by Prof. Pickering at a station half a mile distant from the rest of the party. Using successively an Arago Polariscopes, one of the form employed by Prazmowski, and a Savart, he is understood to have obtained with all three, results pointing to a radial

polarisation of the Corona. The light covering the moon's disc he observed to be polarised throughout in the same plane, and the observations showed that the Arago and other Polariscopes dependent on colour were sufficiently delicate to determine this plane with accuracy.

At the same time Mr. Ross, his assistant, using the instrument employed by Prof. Pickering in the last eclipse, obtained the same results as were then found. Mr. Ross used a modification of the Bunsen photometer, and obtained several concordant measurements, showing that the light was equal to that of a standard candle at two feet.

The writer used a Savart's polariscopes attached to a small telescope of 1½ inches aperture, and having a field of about 2°.

The observations with the Savart's polariscopes being subject to ready misconception, the preparation for observation and the appearance during it are here given with some minuteness.

Before the eclipse the Savart was so adjusted that the bands were most distinct when vertical, viewing the meridian sun reflected from water. None were visible when the sun was directly scrutinised before or after totality.

During totality the appearance which presented itself was unexpected.

The bands were distinctly seen on the corona, and were brightest where normal to the limb and where tangential to it. As the polariscopes were slowly rotated, no marked diminution of their brightness was seen, and when it had been turned through 45° they were as bright as before; distinctly visible even in colour, and they so remained, the rotation being continued for greater security through a whole revolution, during the whole time they presented the appearance described, and characteristic of radial polarisation. They were not noticed on the disc of the moon, but this may well have been from the observer's attention being so exclusively directed to verifying their persistency on the corona.

The writer also employed a good achromatic, of four inches aperture, with a power of about 150, in the direct study of the coronal structure with negative results.

On the closest scrutiny of the part nearest the sun, nothing was seen but a nearly uniform diffuse light, except that one "dark ray" in the field was noticed to be absolutely straight and nearly radial. The outline of the Corona was roughly quadrangular, and a heavy field bar provided for the purpose being carefully set during totality in the direction of the longer diagonal, was found on subsequent estimation to make an angle of as nearly as possible 45° with the vertical.

The "red flames" were beautiful objects, as incidentally noticed during the telescopic scrutiny of the Corona, but not, I may add, more distinctly or more in detail than I had viewed them the day before through the Spectroscope of Professor Young.

The coronal structure is well shown on a photograph obtained by Mr. Willard during totality; a very interesting drawing of it was also taken by Mr. Gordon, a gentleman resident in the vicinity; and all the observers have descriptions to give of its appearance to the naked eye, which differ in some degree from each other. If I compare my impressions with those of others, or even with my own of last year, I find difference enough to suggest the probability of considerable "personality" in all such statements. In some well-marked features all agree, in other minor ones such difference exists that one might almost say each saw a different Corona. The observations with the Spectroscope and Polariscopes are happily more removed from uncertainty.

The conclusions reached with both are not perhaps as absolute as they would have been with the cloudless sky of last year's Eclipse, they are still such as to fully justify, we may hope, the cost of labour and time in obtaining them, and which none of those present can regret.

S. P. LANGLEY